Misleading Tests of Health Behavior Theories

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ABSTRACT

Most tests of cognitively oriented theories of health behavior are based on correlational data. Unfortunately, such tests are often biased, overestimating the accuracy of the theories they seek to evaluate. These biases are especially strong when studies examine health behaviors that need to be performed repeatedly, such as medication adherence, diet, exercise, and condom use. Several misleading data analysis procedures further exaggerate the theories' predictive accuracy. Because correlational designs are not adequate for deciding whether a particular construct affects behavior or for testing one theory against another, most of the literature aiming to test these theories tells us little about their validity or completeness. Neither does the existing empirical literature support decisions to use these theories to design interventions. In addition to discussing problems with correlational data, this article offers ideas for alternative testing strategies.

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INTRODUCTION

Literally thousands of studies of health behaviors (1) describe themselves as either testing or being guided by specific theories, including the health belief model (2), protection motivation theory (3,4), subjective expected utility theory (5,6), the theory of reasoned action (TRA) (7,8), and the theory of planned behavior (TPB) (9,10). Given this enormous effort, one might expect that the determinants of health behaviors would be well understood, but this is not the case. Most of these studies tell us little about the causal factors underlying health behaviors, the completeness of existing theories, or the superiority of one theory over another (1,11).

New theories of health behavior continue to be proposed (12–14). For the most part, they attempt to explain the same types of health behaviors and use many of the same social and cognitive constructs as existing models. However, in the absence of convincing theory tests, the old theories are never abandoned and are seldom even modified (15). It appears that rigorous theory testing, the hallmark of science, is not occurring within the domain of health behavior research.

Probably the most important reason for this state of affairs is the excessive reliance on correlational designs to assess causal relations. For example, most of a sample of 12 recent meta-analyses of health behavior theories (see Table 1) (16–27) included no experimental data whatsoever. The absence of such data is not surprising, because few relevant experiments exist. In fact, a recent meta-analysis of the relation between risk perception and vaccination behavior (28) located 35 correlational studies but not a single intervention. Furthermore, out of the several thousand studies mentioned earlier, Noar and Zimmerman (1) found only 19 that pursued the laudable goal of testing one theory against another. Yet all 19 were based on correlational data, and in 16 cases, the best theory was simply interpreted as the one with the largest multiple correlation coefficient for the prediction of behavior or intentions.

There seems to be an implicit assumption on the part of many researchers that although correlations may sometimes exaggerate or underestimate effects, overall, they give a reasonably accurate picture of how strongly a factor influences behavior.

Problems in using correlational data, whether cross-sectional or longitudinal, to infer causation have already been examined in depth and sophistication by experts in research methodology, statistics, and philosophy of science (29–35). Yet their arguments appear to have had little impact on the designs and analyses employed by health behavior researchers. Possible explanations for this lack of impact include the complex or mathematical nature of the arguments, the presentation of these arguments in general terms without specific reference to health behaviors, the absence of these discussions from publications read by health researchers, and the diverse backgrounds of people who study health behavior.

The purpose of this article is not to repeat the familiar argument that correlation is not causation. Rather, our goal is to demonstrate that the correlations derived from health behavior research using cross sectional and prospective designs have substantial and systematic errors when used to evaluate the effects of independent variables and that these errors usually inflate the apparent accuracy of the health behavior theories that the studies aim to test. The article also explains why these errors should be smaller for some types of health behaviors than others.

In addition, we examine several misleading data analysis practices common in health behavior research, including a reliance on intentions as the outcome criterion and confusion about whether to control for prior behavior in prospective designs.

It is important to emphasize that this article is a critique of the ways health behavior theories are being tested, not an attack on any particular theory. However, because current testing pro-

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TABLE 1
Absence of Intervention Studies in Recent Meta-Analytic Reviews

Review	*Meta-Analysis Topic*[a]	*No. of Data Sets Reviewed*	*Intervention Data Sets Reviewed*
Albarracin, Johnson, Fishbein, and Muellerleile, 2001 (16)	TRA, TPB (condom use)	96	0
Armitrage and Conner, 2001 (17)	TPB	185	0
Floyd, Prentice-Dunn, and Rogers, 2000 (18)	PMT	65	0
Harrison, Harrison, Muller, and Green, 1992 (19)	HBM	17	0
Hausenblas, Carron, and Mack, 1997 (20)	TRA (exercise)	162	0
Marshall and Biddle, 2001 (21)	TTM (exercise)	91	11
Milne, Sheeran, and Orbell, 2000 (22)	PMT	29	11
Notani, 1998 (23)	TPB (behavioral control)	63	0
Rosen, 2000 (24)	TTM (change processes)	47	0
Schulze and Whittmann, 2003 (25)	TRA, TPB	27	0
Sheeran and Taylor, 1999 (26)	TRA, TPB (condom use)	67	0
Witte and Allen, 2000 (27)	PMT, SEU, EPPM (fear appeals)	99	99

Note. TRA = theory of reasoned action; TPB = theory of planned behavior; PMT = protection motivation theory; HBM = health belief model; TTM = transtheoretical model; SEU = subjective expected utility theory; EPPM = extended parallel process model.
[a]Topic is the theory shown unless a more specific issue is also listed.

cedures usually exaggerate the extent to which the theories are capable of explaining health behavior, one major conclusion is that the empirical support for health behavior theories is actually much weaker than commonly recognized.

PROBLEMS INFERRING CAUSALITY FROM CROSS-SECTIONAL HEALTH BEHAVIOR DATA

Effects of Behavior on Perceptions

Theories use a variety of constructs to explain individual health behaviors. Among health behavior theories, cognitive variables (such as beliefs, attitudes, perceptions of self-efficacy, and intentions) receive the greatest attention, perhaps because they appear more amenable to change than social, environmental, cultural, personality, and physiological factors. For convenience, in this article, all these cognitive constructs are called *perceptions*.

In the most common type of investigation, cross-sectional survey data are collected on perceptions and behavior, with the hope that correlations between these constructs can be interpreted as causal effects of perceptions on behavior. In general, the studies find that people who say that a behavior is desirable, effective, beneficial, and not excessively difficult have already been performing that behavior and intend to continue. People who say that a behavior is undesirable, ineffective, not beneficial, or very difficult have not been performing the behavior and do not plan to start. These findings provide support for theories predicated on such perceptions only if the perceptions were the cause of the behavior. But could causality flow in the opposite direction, from behavior to perceptions?

In fact, a vast body of research has established unequivocally that behavior affects perceptions in many ways. Performing a behavior provides new information that can change the actor's perceptions of his or her self-efficacy for this action (36). For example, although a typical study (37) reported a cross-sectional correlation of .67 between self-efficacy for condom use and actual condom use, it is hardly surprising that people who are already using condoms regularly are more confident about their ability to use condoms than people who are only using condoms occasionally. Some large, but unknown, proportion of this impressive correlation undoubtedly reflects the effects of behavior on self-efficacy rather than the effects of self-efficacy on behavior.

More broadly, as expressed by self-perception theory (38), people use their behavior to make inferences about their own beliefs and interests. Thus, people might infer from their failure to take a precaution that they must not be very concerned about the risk. Behavioral experience also provides information that can alter perceptions of the health behavior itself, including its benefits, costs, and difficulty. Finally, people want to believe that their behaviors are wise and appropriate, and, as predicted by the theory of cognitive dissonance (39,40), they will develop reasons post hoc that justify their actions. For example, when people drop out of smoking cessation programs, they lower their ratings of the dangers of smoking, thereby minimizing any dissonance that might be created by their failure to quit (41,42). Clearly, favorable perceptions about a behavior can be created or strengthened by performing that behavior, and unfavorable perceptions about a behavior can be created or strengthened as a consequence of choosing not to perform a behavior.

Thus, behavioral performance tends to produce perceptions supportive of the behavior, but these effects are misclassified by correlational tests as the effects of these perceptions on behavior, the direction proposed by the theories. As a consequence, such correlational tests overestimate the ability of cognitive-oriented theories to explain health behaviors.

Types of Health Behaviors

The effects of behaviors on perceptions are likely to be greater with some types of health behaviors than others. One type can be called *ongoing* or *repeated* behaviors. These are actions that people perform frequently or fail to perform despite having multiple opportunities. Examples include seat belt use, daily food and exercise behavior, sun protection, contraception and condom use (for people who are sexually active), bicycle helmet use, long-term medication adherence, and dental hygiene. In such instances, people generally have a great deal of information concerning their own past actions and often about the consequences of these actions. A second category can be called *intermittent behaviors*. These are behaviors that people have (or have not) performed in the past, but only occasionally, at considerable intervals. Examples include medical and dental checkups, annual influenza vaccination, and colonoscopy. Compared with ongoing behaviors, people's experience with these intermittent behaviors is limited. A third category consists of health behaviors that people are encountering for the first time. These include health actions that are completely new (e.g., the chance to take a new vaccine or a new screening test) and also the first opportunity to take an already known health action (e.g., the first time one's doctor suggests a colonoscopy). By definition, people have no prior personal experience performing actions in this third category.

The need to justify our behavior, practice developing such justifications, and feedback received from actually performing the behavior will be strongest for ongoing behaviors. Thus, with such behaviors, effects of behavior on perceptions are likely to substantially inflate the perception–behavior correlation. However, when a behavior has been performed only a few times, the need to justify having performed it in the past and experience creating such justifications may be weaker, and the argument that correlations between perceptions and behavior may simply reflect the effects of behavior on perceptions is a little weaker. Consistent with these contentions, Notani's meta-analysis (23) found that correlations of perceived behavioral control with intentions and behavior were greater for frequently performed behaviors than for more unfamiliar behaviors.

However, if perceptions predict the adoption of a new behavior, this is much more convincing support for claims of causality because the "behavior causing perceptions" explanation is untenable. Such tests require prospective data, with perceptions assessed before people have had an opportunity to act but not before they have formed perceptions of the health threat and the behavior. Note that the behavior must be new in the sense of a new possibility, not simply the first performance of a behavior that has been available and declined in the past. Examples of studies of new behaviors include the acceptance of an initial offer of genetic testing for cancer susceptibility (43,44), participation by migrant farm workers in a new tuberculosis screening program (45), and adoption of a new vaccine against Lyme disease (46).

PROBLEMS INFERRING CAUSALITY FROM PROSPECTIVE DATA

Effects of Behavior on Perceptions

Effects of behavior on perceptions are just as serious an issue in prospective designs. If favorable perceptions have developed because of the regular performance of a behavior, such as exercise or condom use, these perceptions will be strongly correlated with past behavior. When studying an ongoing behavior, a correlation between perceptions measured at the start of the study and behavior measured later might just reflect the shaping of the initial perceptions by prior behavior and the ability of prior behavior to predict future behavior, with no causal link from initial perceptions to behavior.

If such an indirect link between initial perceptions and future behavior is mistakenly viewed as a direct causal connection, the correlation between the two variables will overestimate the causal role of the perceptions. Many prospective tests of health behaviors rely on such correlations to decide whether the perceptions have causal importance. However, past behavior is often the strongest predictor of future behavior (35,47). Thus, it is quite plausible that a prospective relation between perceptions and behavior does not represent a causal relationship.

Might perceptions be created by behavioral experiences (such as beliefs about action difficulty or effectiveness) and then genuinely sustain or modify future behavior? This is certainly a possibility. The difficulty is in separating this type of direct causal relation from one in which the perceptions do not have any causal role. Another complication is that perceptions based on actual experience influence behavior more powerfully than perceptions derived from other sources (48). Thus, a perception that colonoscopies are not painful is likely to have a stronger influence on future behavior if the perception is based on personal experience than on a doctor's statement. Even if the perceptions created by experience with a behavior do influence subsequent behavior, it would be a mistake to assume that perceptions created by other means—such as a media campaign—would have the same effects.

Prospective Studies and Controls for Initial Behavior

Controlling for initial (or past) behavior might seem to eliminate the problems described in the preceding section. However, this approach introduces new problems that can be understood by looking at relevant path models (49). Most tests of health behavior theories using prospective data simply assume that the correlation between perceptions at one time (P_1) and behavior at a later time (B_2), $\mathbf{r}_{P1B2}$, corresponds to the causal path between these two variables. This path is labeled **a** in Figure 1a. The alternative hypothesis, discussed in the preceding section and shown in Figure 1b, is that $\mathbf{r}_{P1B2}$ has nothing to do with the effects of perceptions on behavior and merely reflects the effects of initial behavior on initial perceptions and the stability of be-

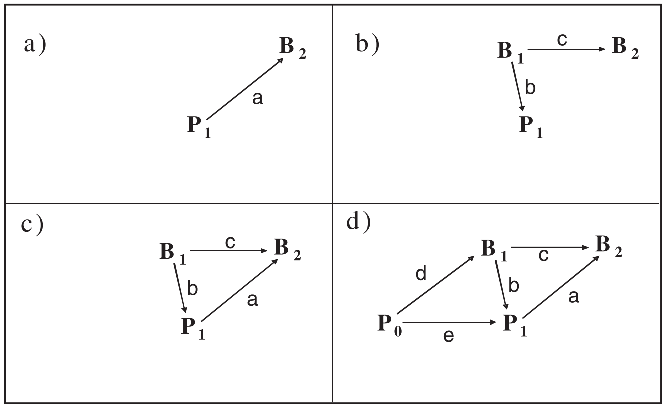

FIGURE 1 Path analyses representing alternative models of the causal links between perceptions (P) and behavior (B). Subscripts 0, 1, and 2 refer to successive times of measurement. Lowercase letters are standardized path coefficients.

havior over time. The combination of these two models is shown in Figure 1c. Applying path analysis (49) to Figure 1c, $\mathbf{r}_{P1B2} = \mathbf{a} + \mathbf{b} \times \mathbf{c}$, where the letters on the right hand side of the equation are standardized path coefficients. Thus, path analysis verifies our earlier claim that effects of prior/initial behavior on perceptions in prospective data contribute to the perception-subsequent behavior correlation. Because **b** and **c** are both expected to be positive, $\mathbf{r}_{P1B2}$ will overestimate path **a**, the path representing the causal impact of perceptions on behavior (i.e., how much B_2 would change if we changed P_1).

Unlike the situation just discussed, some theory variables that refer to a person's present state (e.g., his or her perceived susceptibility or worry), rather than to perceptions about the behavior or hazard, are predicted to have an effect on future behavior (path **a**) that is opposite in sign to the effect of behavior on the variable (path **b**) (47). For example, believing that we are at high risk for heart disease is expected to motivate us to exercise more (so path **a** is positive), but increasing exercise is expected to decrease our perceived risk (so path **b** is negative). With path **c** being positive, $\mathbf{b} \times \mathbf{c}$ will be negative, $\mathbf{a} + \mathbf{b} \times \mathbf{c}$ will be less than **a**, and the $\mathbf{r}_{P1B2}$ correlation will *underestimate* path **a**. More typical, however, is the case in which effects of perceptions on behavior lead to an overestimation of the causal role of perceptions.

It might seem that one can avoid the problem caused by path **bc** by calculating the partial correlation between beliefs and subsequent behavior, controlling for initial behavior (i.e., $\mathbf{r}_{P1B2/B1}$). One would control for current behavior when studying ongoing behaviors and past behavior when studying intermittent behaviors. Prospective studies of new behaviors have no need to control for initial behavior because all study participants are in the same (i.e., no action) state. However, the partial correlation is not the same as path **a** in Figure 1c. For the model in Figure 1c, $\mathbf{a} = \mathbf{r}_{P1B2|B1}\ \mathrm{sqrt}(1 - \mathbf{r}_{B1B2}^2) / \mathrm{sqrt}(1 - \mathbf{r}_{B1P1}^2)$.

Even if one used the correct equation to calculate path **a**, there is still a serious problem. An analysis of path **a** based on the model in Figure 1c determines how much perceptions at Time 1 can predict Time 2 behavior *beyond what could be predicted by behavior at (or prior to) Time 1*. Thus, this analysis focuses on the ability to predict behavior *change* between Time 1 and Time 2. In situations in which perceptions are changing, such an analysis might be quite informative, but explaining why behavior may have changed during this particular time interval is only a part of the story of what has led to the behavior.

To understand this subtle point, consider the following example. Imagine that a doctor tells a young patient at Time 0 that because of her chronic health condition she needs to get an annual flu shot. The doctor's explanation causes the patient to get a flu shot when fall arrives. The next year, for the same reason, she gets another flu shot. Other young people do not think they need a flu shot and do not get one. As seen in Figure 1d, the young patient's perceptions after seeing the doctor at Time 0, denoted P_0, are the cause of her subsequent behavior B_1. Her perceptions about the need for a shot remain stable, so Time 0 perceptions also determine the perceptions observed later, P_1. The rest of the diagram is the same as Figure 1c. In effect, we have simply taken Figure 1c and added an explanation of how B_1 comes about.

Now imagine starting a study at Time 1 with the goal of understanding the causes of flu vaccination. The correlation between Time 1 perceptions and vaccination behavior measured at Time 2 will be very large. But, with such stable behavior, Time 1 perceptions do not improve the prediction of Time 2 behavior or beyond what can be predicted from past behavior, so path **a** will be very small. Looking only at the path analysis, we would conclude that perceptions are not important and that an intervention that altered perceptions would not increase vaccination. However, such conclusions would be completely wrong. Perceptions were the cause of vaccination. The reason why the path analysis led us to the wrong conclusion is that it focused exclusively on changes on behavior between Times 1 and 2 and ignored the causes of Time 1 behavior.[4]

Looking solely at path **a** ignores the effect on B_2 that flows from P_0 through B_1 (i.e., **dc**). With prospective data, controlling for past behavior is overly conservative, understating the total effect of perceptions on behavior. However, as discussed earlier, failing to control for past behavior is also likely to be misleading, overstating the effects of perceptions on behavior.

The same problem applies to investigations of diet, exercise, flossing, condom use, sun protection, bicycle helmet use, or other relatively stable ongoing health practices. We might measure perceptions and behavior at an initial point in time, measure behavior again some time later, and then calculate the path from perceptions to sequent behavior. But, unless some impactful event has altered the situation, changes in behavior between the two measurements are likely to represent small, random fluctuations—such as measurement error or transient changes in life circumstances—unrelated to preexisting perceptions. As a consequence, we would expect to find that risk perceptions do not improve the prediction of future behavior. This

[4]This analysis corrects a previous discussion (66) that had recommended controlling for current/past behavior when examining the prospective relationship between risk perceptions and future behavior.

does not prove, however, that perceptions do not affect behavior. Chapman's (50) influenza data show the pattern just described. Most perceptions about influenza vaccination were very stable from year to year. Some of the perceptions she studied significantly predicted subsequent vaccination behavior, but these predictions became nonsignificant after controlling for the previous year's vaccination.

What if some event—a celebrity endorsement, media report, doctor's advice, or friend's illness—has just occurred that may have changed perceptions? Would it be possible to see whether changes in perceptions produce changes in behavior in this situation with correlational data? Yes, but to observe the consequences of this event, one must (a) assess the perceptions that existed before the event, (b) reassess perceptions after the event but before people have had time to make any changes in behavior, (c) assess behavior before the event (or so soon after it that people have not had time to change their behavior), and (d) reassess behavior long enough after the change in perceptions for behavior to have changed (35). These four sets of data allow one to determine whether changes in perceptions predict subsequent changes in behavior, which would suggest a causal relation. Collecting such data would obviously be very difficult unless one were to know in advance when such an event would occur.

PROBLEMATIC RESEARCH DESIGNS

The preceding arguments about the analysis and interpretation of cross-sectional and prospective designs are summarized in Table 2. Each cell is labeled to indicate problems in the ability to infer an effect of perceptions on behavior from the correlation indicated. In Table 2, *most likely* refers to situations in which the correlation is most likely to be inflated by the effects of behavior on perceptions. The *most likely* category occurs with theory tests that do not require any change in behavior and merely reflect perception–behavior agreement (in cross-sectional or prospective data) for ongoing behaviors. The label *likely* occurs with theory tests that concern perception–behavior consistency for intermittent behaviors. One might think of the *likely* tests as examining the readoption of a behavior, in contrast to the most problematic tests which concern the continuation of an already existing pattern of behavior.

Prospective studies that examine only the subset of people who have not yet adopted a healthy behavior despite opportunities to do so (e.g., studies of smokers or of people who do not exercise) belong in the category of studies that control for initial behavior in Table 2. Choosing such a subset of the population is similar to controlling for past or initial behavior in that initial behavior is not a confounding variable because it is constant within the subset.

Least susceptible to the biases under discussion are prospective studies that examine changes in levels of behaviors or the adoption of new behaviors.

Note that all the designs in Table 2 are problematic as tests of the effects of perceptions on behavior. The choice is not between correct and incorrect correlational designs but between ones that are more and less misleading. With ongoing behaviors studied prospectively, one has the unsatisfactory choice between not controlling for prior behavior, which usually overstates the effects of perceptions on behavior, or controlling for prior behavior, which is likely to underestimate these effects. The least misleading strategy may be to report both results.

APPROPRIATE USE OF CORRELATIONAL DATA IN HEALTH BEHAVIOR THEORY TESTING

Clearly, correlations from both cross-sectional and most prospective research designs reflect effects of behavior on perceptions as well as effects of perceptions on behavior. These correlations are also influenced by other familiar methodological problems, including correlated measurement error and the always-present possibility that an association may exist only because of the shared impact of a third variable (50). Because the magnitude of these perception–behavior correlations does not tell us anything clear about the presence or size of causal effects, what uses do they have?

TABLE 2
Perception–Behavior Correlations Likely to Be Biased by Effects of Behavior on Perceptions

	Correlation Examined/Likelihood of Bias		
Research Design	*Ongoing Behaviors*[a]	*Intermittent Behaviors*[b]	*New Behaviors*[c]
Cross-sectional/retrospective	Current perceptions with current/ recent behavior / *Most likely*	Current perceptions with recent behavior / *Likely*	NA (no behavior yet performed)
Prospective (analysis does not control for past or initial behavior)	Current perceptions with future behavior / *Most likely*	Current perceptions with future behavior / *Likely*	Current perceptions with future behavior / *Least likely*
Prospective (analysis controls for past or initial behavior)	Current perceptions with future behavior, controlling for present behavior / *Least likely, but underestimates effects of perceptions on behavior*	Current perceptions with future behavior, controlling for past behavior / *Least likely but underestimates effects of perceptions on behavior*	NA (no initial precaution so controls are not possible)

[a]Behaviors that have been performed many times and are still continuing. [b]Behaviors that have been performed a few times. [c]Behaviors that have not been available previously to be performed.

One might argue that the ability to predict behavior is valuable even if successful prediction cannot be equated with causation. Seldom, however, is prediction alone particularly helpful. It may be interesting to know that one subgroup of the population is less likely to engage in healthy behavior than another, suggesting that this subgroup needs extra encouragement, but we still need cause and effect information to decide what kind of encouragement will be effective (35).

The correlations assessed in most health behavior research might best be viewed as pilot results that can help set priorities for adequately controlled experiments. Variables that predict behavior well should have a higher priority for future experimental study than variables that predict poorly. Theories that predict well should have a high priority for experimental verification than theories that predict poorly. If one way of assessing a variable predicts better than another measurement approach, the former should have first priority for additional study.

What one should not do is use the correlations derived from cross-sectional or prospective research to decide whether a variable has a significant causal effect, to decide which variable has the strongest effect on behavior, or to decide which theory offers the best explanation of behavior. Such mistaken conclusions are the norm in correlational research on health behavior, and they have given us unwarranted confidence in the validity of current theories.

TESTING HOW WELL HEALTH BEHAVIOR THEORIES PREDICT BEHAVIOR

If knowing the ability of a variable or theory to predict behavior can help set research priorities, it is important to think carefully about the types of predictions that are most relevant. If our interest is in changing health behaviors, two issues deserve particular consideration.

Overemphasis on Individual Links in Theories

One aim in testing health behavior theories is to investigate the process specified by the theory. The researcher might ask whether the paths corresponding to each causal link in the theory, such as a link between constructs A and B are significant. A different aim is to determine how well the theory predicts B. If the theory provides an excellent prediction, perhaps it has identified all the main causes of B. Goodness of fit measures (49) used in structural equation modeling indicate whether the observed pattern of associations among variables is consistent with the pattern predicted by a theory or whether the pathways in the model should be changed (i.e., aim one), but the a goodness of fit statistic can be very large even if the associations themselves are weak and behavior is poorly predicted.

Many tests of health behavior theories emphasize the first goal, testing the separate links in the theories. In the TRA, for example, this would include links from behavioral beliefs (i.e., beliefs about the value and likelihood of the behavior's consequences) to attitudes, from attitudes to intentions, and from intentions to behavior. One might expect that knowing the strengths of the separate links is sufficient to show whether the overall belief– behavior link is also significant. This is not correct. Even if the separate links are each quite strong, the overall association between beliefs and behavior is not determined and can take on a surprisingly large range of values.

To illustrate this point, consider a simple theory that states that perceptions, P, produce intentions, I, and intentions produce behavior, B. Assume that we have measured r_{PI} and r_{IB}. Knowing the correlations for these two separate links, what can we say about the correlation between perceptions and behavior? If we assume that the theory is correct (i.e., that intention completely mediates the effect of perceptions on behavior), then $r_{PB} = r_{PI}r_{IB}$. If a study reports a perceptions–intentions correlation of .6 and an intentions–behavior correlation of .6 (values resembling those reported in some meta-analyses for TRA), then r_{PB} will be .36 in our example. In this case, the behavioral perceptions P would explain 13% [i.e., $(.6 \times .6)^2$] of the variability in B. With 87% of the variability in behavior unpredicted, we would surely look for other predictors.

But what if we do not assume that the theory is correct? Does knowing how two variables correlate with a third variable (i.e., P with I and I with B) tell us how they correlate with one another (i.e., P with B)? In fact, all we can say for certain is that r_{PB} must fall somewhere into the range $r_{PI}r_{IB} \pm [1 - r_{PI}^2 - r_{IB}^2 + (r_{PI}r_{IB})^2]^{1/2}$ (52, p. 280).

According to this equation, the perceptions–behavior correlation in our example could fall anywhere in the broad range between –.28 and 1.0. In other words, knowing the separate links gives us no assurance that the distal or exogenous constructs in the theory—typically the ones we would try to change by our interventions—are able to predict behavior at all! Tests of health behavior theories should always report how well the perceptions—both separately and in combination—predict behavior, not just how well they predict intervening variables. Research on behavioral intentions is no substitute for research on actual behavior.

Excluding Intentions and Other Intervening Variables From the Prediction Equation

When authors ask, "How good are a theory's predictions?" they often look only at prediction by intervening variables that are close to behavior, such as intentions or attitudes. Especially with TRA and TPB, researchers may never even report the degree to which many theory components, such as behavioral beliefs, expectancies, normative beliefs, and motivation to comply, predict behavior. In fact, for TRA and TPB, the multiple correlation coefficient for predicting behavior from attitudes and subjective norms (plus perceived behavioral control in the case of TPB) is the criterion by which the validity of the model and its overall predictive accuracy are frequently judged (17,25).

In those studies that do use more distal perceptions in predicting behavior, intentions are usually also included in the multiple regression analysis as an independent variable (e.g., 53–56). (Many discussions of the health belief model [e.g., 53,56,57] and of protection motivation theory also assume that intentions are the immediate cause of behavior, so this discussion applies to these theories as well.) Such an analysis is useful to test the assertion that the effects of distal beliefs are completely mediated by intentions. If these distal beliefs improve the prediction beyond that provided by intentions, the assertion is disconfirmed. Often, however, such analyses are not intended to test the mediational model prescribed by a theory but to determine how well the theory predicts behavior. When used this way, the analyses are very misleading.

There is more than one way to determine predictive accuracy, and it may help to illustrate these ways with a specific theory, TRA. According to this theory, behavior should be completely predicted by intentions. A regression analysis to determine how well intentions predict behavior is one way to see how well the theory predicts behavior. However, according to TRA, intentions are completely determined by attitudes and subjective norms, so these two variables constitute a second, sufficient set of predictors, and this gives us a second way to test the theory's predictive ability. Finally, because the theory claims to specify all the causes of attitudes and subjective norms, these causal variables—a third, most distal set of variables—make up another sufficient set of predictors.

Explicit in the TRA's prescribed sequence of causal links is the idea that to change behavior one must change the most distal predictors: the behavioral beliefs, outcome expectancies, and so forth. According to the TRA, there is no other way to change attitudes, subjective norms, or intentions. If intentions predict behavior quite well, but the theory is wrong about the causes of intentions, then the theory does not give one any way to change behavior and it would be so incomplete that it would not be very useful. If we want to find out the extent to which the TRA has identified most of the predictors of behavior and if our focus is on behavior change rather than the model's internal structure, intentions and other intervening variables should not be included in multiple regression analyses. Assuming good measurement of constructs, inaccuracies in predicting behavior would suggest that additional variables need to be added to the theory. (See Sutton [35,58] for a discussion of these issues. He refers to the prediction of behavior from distal variables as the "effective variance explained.")

An example may clarify why mediating variables, such as intentions, should be excluded from analyses with these aims. Suppose that our goal is to develop a theory that explains the causes of heart attacks. Our theory asserts that chocolate causes blocked arteries and that blocked arteries cause heart attacks. Assume that a blocked artery is an excellent predictor of a heart attack—the two variables are highly correlated—and assume further that this relationship is causal. Our theory is very successful at one level because it does identify the immediate cause of heart attacks, but it is not a sufficient explanation of heart attacks unless it is also correct about what causes blocked arteries. Even if chocolate has nothing to do with blockages, a multiple regression equation containing both chocolate consumption and blockages (the intervening construct) will predict heart attacks extremely well, simply because a blockage is such a good predictor. To conclude, however, on the basis of the large multiple regression coefficient, that this theoretical model provides an adequate explanation of the determinants of heart attacks—and, especially, to accept the corollary that avoiding chocolate will substantially reduce heart attack risk—would clearly be incorrect. Furthermore, a much better theory that identified the real causes of arterial blockages would not be any better at predicting heart attacks, so long as the blockage variable itself remains in the prediction equation.

As the preceding example shows, a multiple regression analysis used to provide an overall test of a theory of health behavior can be quite misleading if it includes intervening variables that cannot be modified directly. Because intentions are usually associated more closely with behavior than any other variables in these theories, removing intentions from a prediction equation will generally decrease the ability of the theory to predict behavior, perhaps by a great deal.

If a theory includes a variable without specifying its determinants—in other words, the theory acknowledges that the variable is determined by constructs not identified by the theory—it is appropriate to include the variable in a regression test of how well the theory predicts behavior. However, the theory is less complete and less useful—even if it predicts behavior well—if it does not provide insights into the determinants of its constituents.

Similarly, from an intervention perspective, to decide whether perceived behavioral control is a useful improvement to the TRA, one should compare the improvement in predictions provided by this variable to the improvement in predictions provided by other individual distal variables. For example, even if perceived behavioral control improves predictions of behavior by only a few percent, as Armitrage and Conner (17) concluded, this may be as important a contribution as any other single variable that an intervention might be able to alter.

ALTERNATIVES

The obvious solution to the problems described here is to conduct fewer surveys and more experiments or quasi-experiments (33,55,58,59). Some important issues, such as the effects of poverty on health or the effects of family dynamics on child personality are nearly impossible to study through experimentation. Interventions to test cognitively oriented health behavior theories, in contrast, are relatively straightforward, although considerably more difficult than correlational research. Random-assignment experiments in which theory constructs are each manipulated separately yield the least ambiguous cause and effect conclusions, but carefully chosen quasi-experimental designs can usually eliminate most threats to internal validity (33,59). Although theory-testing experiments are growing in number (57), they still represent a very small fraction of health behavior research. Greater emphasis on interventions would also address one of the main complaints of applied researchers, namely, that theories of health behavior are often unhelpful because they say nothing about how to modify the constructs in the theories.

A clear attraction of correlational research is the illusion of testing an entire theory in a single study (i.e., the effect on behavior of all the theory's constructs). For reasons of feasibility, experiments and quasi-experiments are only able to test the causal impact and interactions of a very small number of constructs, typically only a portion of a theory. Still, with carefully chosen variables and designs, experiments can focus on the differences among theories (11) and help identify the ones that are superior, which would represent major progress.

Even multi-faceted interventions whose goal is to promote healthy behavior, rather than to test theories, can provide information that informs theory. However, to serve this role, researchers need to specify the constructs that are hypothesized to affect behavior, measure these constructs before and after the intervention has been applied, and conduct mediational analyses to determine whether these constructs may have served as the vehicles of change (60–62). Very little applied health research makes this extra effort, and, therefore, few applied studies provide any information about why they succeed or fail.

An informative, nonexperimental approach would be to conduct prospective studies in situations where new behaviors have become available or where perceptions have undergone significant changes (e.g., 43–46). These opportunities may result from naturally occurring events (e.g., the discovery of a new infectious disease or the publicity surrounding the illness of a television personality) or they may be created by investigators. Thus, investigators might invite people to a health screening, offer parents free child auto safety seats, inform homeowners how to test their water for lead, or provide low-cost pneumonia vaccine to seniors and then study how people respond. So long as these actions are unfamiliar, the behavior-to-perception problem that plagues most correlational research on health behaviors is avoided.

CONCLUSION

As Table 2 indicates, there are few situations in which correlational data give clear insights—let alone proofs—concerning the causes of health behaviors. Unfortunately, most theory-related studies of health behavior fall into the "most likely to show bias" cells of Table 2. Correlational studies of reactions to new precautions have the fewest problems, but they make up a tiny portion of current research.

The growing number of meta-analyses in the health behavior literature may only worsen the situation. Consolidating numerous correlational studies in which relations are ambiguous or biased does not eliminate these faults (63). Some authors of the meta-analyses in Table 1 are careful to use the language of prediction rather than causation in discussing their results, but only Milne, Sheeran, and Orbell (22) devoted more than the briefest mention to the issue of causal inference. Several others, in contrast, use these meta-analyses to support their claims that the theory in question has been validated and should be used to guide individual and community interventions (16,18).

Because they conflate effects of perceptions on behavior with effects of behavior on perceptions, the magnitudes of the effect sizes calculated by such meta-analyses are not very meaningful. Instead of striving to arrive at precise estimates of these associations, meta-analyses should aim to identify those constructs that consistently improve behavioral predictions, so that they can be studied experimentally. The relative magnitudes of prediction (of one construct vs. another or of one theory vs. another) could be used to set priorities for experimentation, but they should not be used to decide which construct has a stronger impact on behavior or which theory is superior.

Hopefully, the preceding arguments will encourage researchers to learn more about design limitations, longitudinal data analysis, and causal inference (29–35,64,65). Editors and reviewers should accept correlational studies for publication only if their goal is appropriate for such designs (e.g., to test the ability of a new construct to improve predictions of behavior), not if they claim, even implicitly, to be testing causal relations.

Correlational studies have an important, but limited, place in theory development. Forcing authors to acknowledge explicitly the limitations of such studies should encourage more experiments. Even a small shift away from correlational designs would be beneficial, for without such a shift, it is doubtful whether there will be any real progress in understanding health behavior.

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